economics & reality

Tony Lawson

ROUTLEDGE

ECONOMICS AND REALITY

Contemporary academic economics, a project often acclaimed as the most scientific of the social sciences, is in a state of some confusion. Its 'theoretical models' are increasingly found to be irrelevant to real world matters, its empirically based forecasting models do not forecast particularly well, and those who provide commentaries on the discipline report a significant disparity between the practices of economists and the 'methodologies' they profess to draw upon. Many who reflect seriously on the situation conclude that, despite widespread pretensions to the contrary, there are few grounds for supposing that economics can be a science at all.

In an innovative and wide-ranging contribution, Tony Lawson concludes that at the root of these problems is a failure of economists to develop methods that are appropriate to their subject matter. In taking steps to compensate, the author demonstrates that a radical transformation in the basic practices and overall direction of the discipline is warranted. In making his case, however, Tony Lawson parts company with many recent social theorists in contending that an efficacious, and indeed emancipatory, science of economics is entirely feasible. In fact, he concludes that economics can be a science in precisely the sense of natural science. However, he establishes that for this possibility to be realised economists must, at a minimum, show a far greater sensitivity to context and adopt significantly broader, more critical, orientations to theorising and explanation than are currently in evidence.

Although informed by philosophy, *Economics and Reality* is rooted throughout in the problems, practices and writings of economists. Exploring the current state, possibilities and limits of economics, the book will be of interest to researchers, teachers and students of economics alike.

Tony Lawson is a member of the Faculty of Economics and Politics at the University of Cambridge. He has published extensively in books and journals including *The Review of Economic Studies*, the *Cambridge Journal of Economics*, *Oxford Economic Papers* and the *Economic Journal*.

ECONOMICS AS SOCIAL THEORY

Series edited by Tony Lawson

University of Cambridge

Social theory is experiencing something of a revival within economics. Critical analyses of the particular nature of the subject matter of social studies and of the types of method, categories and modes of explanation that can legitimately be endorsed for the scientific study of social objects, are re-emerging. Economists are again addressing such issues as the relationship between agency and structure, between the economy and the rest of society, and between inquirer and the object of inquiry. There is renewed interest in elaborating basic categories such as causation, competition, culture, discrimination, evolution, money, need, order, organisation, power, probability, process, rationality, technology, time, truth, uncertainty and value, etc.

The objective for this series is to facilitate this revival further. In contemporary economics the label 'theory' has been appropriated by a group that confines itself to largely a-social, a-historical, mathematical 'modelling'. *Economics as Social Theory* thus reclaims the 'theory' label, offering a platform for alternative, rigorous, but broader and more critical conceptions of theorising.

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For my parents

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PREFACE AND ACKNOWLEDGEMENTS

'No reality, please. We're economists!' This heading to a couple of commentaries in a recent edition of *The Times Higher Education Supplement* (25 March 1994) captures an increasingly widely held perception. According to it, modern economics is largely irrelevant to an understanding of real world matters. Needless to say this view is not accepted by all economists. And it does seem *prima facie* implausible as a characterisation of an academic discipline that is often represented as a social *science*. Yet, my assessment set out below is that it is not wholly mistaken; there *is* a sense in which social reality is neglected in much of modern economics. I shall indicate furthermore how this neglect is debilitating of the subject, with unfortunate repercussions for the multitude of further activities that draw upon it.

My purpose here, however, is not merely to identify limitations of the discipline but also to work towards transcending them. My overall aim, if somewhat unfashionably, is to bring reality (or more of it) back into economics. Of course, in formulating my goal in this way, and in acknowledging some validity to the perception of the discipline noted above, I am not wanting to suggest that modern economics is devoid of a conception of reality altogether. Indeed, it could not be. For the adoption of any method, practice or goal, presupposes a world-view, even if it is left implicit and unexamined. The feature of modern economics which is remarkable here is the minimal concern that is shown either for tailoring methods to insights available regarding the nature of the social world, or for explicitly determining the sorts of conditions under which chosen methods would be appropriate. This is despite a dearth of successes according to criteria which the discipline sets itself. Rather, methods and procedures are formulated according to the nature or degree of their technical sophistication, or their conformity with *a priori* conceptions of proper practice, or some such. The majority of economists seem merely to take it for granted that if ever real world problems or situations are to be studied the chosen methods will necessarily 'fit'. It is in this sense that reality is neglected in modern economics. And we will see that it is just

the presumption that methods of economic analysis can be fashioned without explicit regard to the nature of social phenomena that accounts for the subject's numerous failings.

In short, contemporary economics is marked by an effective neglect of *ontology*, by a lack of attention to elaborating the nature of (social) being or existence. And the project reported here is motivated by a desire to compensate for this neglect. The objective is to determine a sustainable account of natural and social being with a view both to explaining and resolving numerous problems which beset the discipline and to gaining an informed vantage-point on the sort of practices that are likely to bear fruit. Theories which address these sorts of concerns and involve commitments to the reality of features elaborated are usually collected under the heading of philosophical (or ontological) *realism*. The position sustained in this book, then, is an explicitly realist one. The book constitutes a realist theory of and for economics.

Its context can be briefly sketched. In contrast to much contemporary work on economic methodology, the project reported here did not emerge from a formal study of philosophy. Having come to economics by way of first studying mathematics I was immediately impressed by, as I saw it, the widespread and rather uncritical application of formalistic methods and systems to conditions for which they were obviously quite unsuited. In consequence, my interests turned fairly quickly to questions of ontology, and specifically to the study of how methods and modes of reasoning might be fashioned to insights concerning the nature of social being. However, I first approached these issues by way of reading economists rather than philosophers. Given my immediate conviction that the conventional economic methods and procedures are rather obviously inappropriate to the successful investigation of social reality, I assumed that many other economists would have reached the same conclusion long before. I thus supposed they would already have investigated and responded to the situation. In any case, it was an understanding of the economics context specifically that was of primary interest to me.

It turned out that it is mostly (but not exclusively) economists regarded as non-mainstream who have reflected on these matters to any significant extent. I refer here to the likes of Keynes, Hayek, Marx, Dobb, Veblen, Marshall, Smith, Shackle, Menger, Boulding, and Kaldor. In consequence, it is their writings that provided much of the initial background material for the central argument set out below.

In fact, they contribute rather more than this. In one form or another, they already express many of the fundamental tenets of the basic thesis. For example, when Keynes writes that:

If we were dealing with... independent atomic factors and between

> them completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling the laws of their action.
>
> (Keynes, 1973b: 286)

and Marx observes that individuals:

> make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given, and transmitted from the past.
>
> (Marx, 1977: 300)

and Veblen suggests:

> The growth and mutations of the institutional fabric are an outcome of the conduct of the individual members of the group, since it is out of the experience of the individuals, through the habituation of individuals, that institutions arise; and it is in this same experience that these same institutions act to direct and define the aims and end of conduct.
>
> (Veblen, 1909: 243)

and Hayek adds that:

> different rules apply to different individuals according to age, sex, status, or some particular state in which each individual finds itself at the moment.
>
> (Hayek, 1967: 68)

each is expressing in condensed form an important aspect of the thesis which follows. But what a lot of fleshing out and unpacking each assessment requires.

When I eventually turned to formal philosophy, I naturally sought out the writings of philosophers who have been especially concerned with ontological or metaphysical questions. The influence of this literature on what follows appears to distinguish further my project from most other contributions in economic methodology. For although the latter contributions tend to be heavily rooted in the philosophy of science literature, it is questions of epistemic appraisal (i.e. epistemological questions concerning the rational basis for accepting or rejecting theories) that have dominated the economic methodology discussion. This discussion draws primarily upon the philosophies of Popper, and of others like Lakatos who responded to Popper's work (see contributions to Backhouse, 1994, and for a useful overview see Boylan and O'Gorman, 1995). Mäki (1993a: 5) sums up this situation by suggesting that

'A *Popperian dominance*, a kind of Popperian mainstream in economic methodology has prevailed'. It is not clear to me that this emphasis upon epistemological matters, often more or less detached from explicit ontological theorising, has been overly fruitful. Indeed, I feel inclined to support Mäki's assessment 'that the Popperian dominance has led to a misallocation of intellectual resources in economic methodology' resulting in a neglect of, amongst other things, a questioning of 'the nature of metaphysical commitments involved in actual research practice' (1993a: 6). This book, then, can also be seen as an attempt to redirect the methodological debate in economics.

Even so, any philosophical position, including an ontologically oriented one such as defended below, ultimately bears epistemological implications. In consequence, some readers may still feel some surprise to learn that Popper and/or Popperian (epistemological) themes and categories figure relatively little – significantly less than in comparable texts concerned with economic methodology. This 'omission', moreover, may appear especially surprising in the light of the fact that Popper openly proclaims himself a realist, falibilist, and anti-positivist, and recognises the possibility of demarcating science from non-science – where all these are positions openly accepted in what follows.

The basic reason for it is that I not only adopt a more explicit focus upon ontological issues but also defend a theory of ontology which is very different to that presupposed by Popper, at least in his most familiar writings on falsificationism (as opposed to his relatively recent (1990) rather general remarks upon propensities). In consequence, the epistemological questions and implications are correspondingly different. In parallel with Popper I am certainly concerned to criticise various results of the (untenable) philosophical perspective of positivism. But although Popper (1992) claims responsibility for 'killing' positivism, or at least the *logical positivism* of the Vienna circle, there never was a single fatal blow. Positivism, including logical positivism, rests upon at least two fundamental assumptions. Not only does it suppose that science is essentially *monistic* in its development, it also assumes that it is *deductive* in structure. If Popper's contributions to the theory of knowledge, and in particular his work on falsificationism or 'deductivism' (Popper, 1992: 81), have helped undermine the former aspect, these same writings (at least as most have interpreted them) leave the assumed deductivist structure untouched. And it is the presumption of a deductivist structure of science, including its implicit account of *being*, or ontology, that I am concerned here to criticise before anything else.

I should immediately add, however, that if the falsificationist or deductivist programme usually associated with Popper is rejected here as a generalised way of proceeding, Popper's explicit commitment, in that programme, to the pursuit of objective truth and rationality in

science is endorsed. Indeed, at a time when so many economists appear to do little more than flit from fashion to fashion; when, with one or two notable exceptions (e.g. Mäki, 1988, 1992a, 1992b, 1994), economists rarely even mention (let alone accept) the goal of pursuing truth; and when, despite everything, many mainstream economists persist in claiming the authority of Popper for what they do; it seems worthwhile restating Popper's views on these matters explicitly:

> I guess, indeed, that it is the suppressed sense of our own fallibility that is responsible for our despicable tendency to form cliques and to go along with whatever seems to be fashionable: that makes so many of us howl with the wolves. All this is human weakness, which means it ought not to exist. But it does exist, of course; it is even to be found among some scientists. And as it exists, we ought to combat it; first in ourselves, and then, perhaps, in others. For I hold that science *ought* to strive for objective truth, for truth that depends only on the facts; on truth that is above human authority and above arbitration, and certainly above scientific fashions. Some sociologists fail to understand that this objectivity is a possibility towards which science (and therefore scientists) should aim. Yet science *has* aimed at truth for at least 2,500 years.
>
> (Popper, 1990: 34)

There are a number of philosophers (Harré, Hesse and Toulmin) who have focused their criticism on the supposition that science is deductive in structure, and whose writings have had an influence on what follows, just as there are philosophers other than Popper (Bachelard, Lakatos, Kuhn and Feyerabend) who have attacked the positivistic view of scientific development as monistic. But in my view it is Bhaskar (1978, 1979, 1986, 1989) who has managed most successfully to combine the insights of *both* tendencies in constructing a sustainable ontology for science, and whose project has in consequence proved most useful for my own. Although I only came across it in the late 1980s it has had a very significant impact upon what follows.

Nevertheless, I should stress that even Bhaskar's work is not explicitly surveyed here. Influential though Bhaskar's writing has been, the present book does not constitute an introduction to it, or anything like it; for that the reader can consult not only Bhaskar's own contributions but also Andrew Collier's (1994) useful introduction and appraisal. In structure and content the current book is shaped by the situation in economics. As Bhaskar somewhere explicitly acknowledges, his ideas and terminology are not consistent in the sense of being fixed over time. Rather they reveal a developmental consistency: as new problems are resolved, wider concerns are taken on board and earlier conclusions and terminology are revised accordingly. In intent, at least, any consistency to

be found below with Bhaskar's contribution is also of this developmental sort.

This book, then, is about economics, its current state and real possibilities and limits. Although the thesis which follows may be interpreted as broadly philosophical, it takes its premises from, and so is firmly rooted in, problems, practices and writings of economists. The intended readership is, above all, economists, especially those prepared at least to question received doctrines. The central argument which follows does have a wider relevance, but the book was written with the economics community in mind. It is the product of a continuing dissatisfaction with the dominant orientation of the economics discipline combined with a conviction that it is possible to do significantly better.

A brief overview of the argument can be provided by summarising each part of the book.

Part I In Chapter 1 I initiate the discussion through drawing attention to various problems and tensions which pervade economics. In Chapter 2 the widespread acceptance of a particular 'deductivist' conception of science and explanation ultimately rooted in, or at least parasitic upon, positivist results is indicated, and an alternative 'transcendental realist' conception is outlined. The relative superiority of the latter over the former is demonstrated in Chapter 3. In Chapter 4 the problems and tensions of contemporary economics which are noted and described in Chapter 1 are shown to derive from the widespread reliance upon the deductivist conception of science and explanation, while the superior transcendental realist account is found to be free of such problems. In Chapter 5 the reasoning used to establish these results is examined in some detail. Further implications of the perspective supported are elaborated in Chapter 6.

Part II With the central argument broadly laid out in Part I, the object of Part II is to examine in greater detail the various ways in which mainstream economics turns on the deductivist conception of science and explanation and to indicate the sorts of problems that arise in each case. The focus is upon econometrics in Chapter 7; upon 'economic theory' in Chapter 8; and, in Chapter 9, upon those variations on the latter which attempt to play down the need to explain (yet) any actual phenomena. In Chapter 10 I show that the lingering effects of positivism even shape Hayek's early, and currently influential, attempt to develop a 'subjectivist' alternative to the mainstream approach. The essential characteristics of the various projects considered in Chapters 7 to 10 are summarised in Chapter 11, along with conceivable alternatives which also fail to abandon positivist restrictions.

Part III A basic position sustained in Part I is that there are benefits to continually modifying methods of analysis in the light of insights gained into the nature of any given object(s) of enquiry. With this in mind, the focus of Part III is upon social being, and specifically upon the nature of human agency, social structure and their irreducible mutual interdependency. While Chapter 12 is concerned with social structure Chapter 13 concentrates on human agency, including aspects of subjectivity and the nature of human rationality.

Part IV With a conception of social being set out in Parts I and III, the various consequences for economic scientific practice are elaborated in Part IV. In Chapter 14 I draw out implications concerning the basic orientation of economic explanation, and in Chapter 15 I discuss how competing explanatory theories of social phenomena can be assessed. In Chapter 16 I focus upon the much neglected yet fundamental issue of scientific abstraction. The epistemic status of explanatory results is considered in Chapter 17 and the possibility of objective truth is assessed. Chapter 18 constitutes an illustrative example of the explanatory approach defended.

Part V The final part of the book rounds off the argument. In Chapter 19 I trace out the implications for questions of policy and intended change and in Chapter 20 I draw together the various threads of the argument by way of examining the consequences for the enterprise of economic prediction. The basic overall conclusion is that while the academic discipline stands in need of radical transformation, involving amongst other things the abandonment of many of its most cherished ways of proceeding, the possibility of realising an efficacious and emancipatory science of economics is far from being out of the question. Such an achievement, though, will inevitably involve a greater sensitivity to context and a rather more reflective and critical orientation than are currently in evidence.

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The final task that awaits me at this point is the pleasant one of acknowledging at least some of those who have contributed to the preparation of this book over its numerous stages. First of all I must thank those who have helped me to type it. I am ashamed to admit that it was only very recently that I learnt to use a 'word processor'. Thus although I actually managed to type recent versions myself, the project has been under way for sufficiently long that various people have typed early drafts of chapters. In particular I thank Louise Cross, Nacirah

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'Realism and Hayek: A Case of Continuous Transformation', in M. Colonna, H. Haggemann and O.F. Hamouda (eds), *Capitalism, Socialism and Knowledge: The Economics of F.A. Hayek, Vol. II*, 1994, Cheltenham: Edward Elgar.

'Critical Realism and the Analysis of Choice, Explanation and Change', *Advances in Austrian Economics*, 1994, Vol. 1, No.1, pp. 3–30.

'The Nature of Post Keynesianism and its Links to other Traditions', *Journal of Post Keynesian Economics*, 1994, Vol. 16, No. 4, pp. 503–538. Reprinted in D.L. Prychitko (ed.), 1996, *Why Economists Disagree: An Introduction to the Contemporary Schools of Thought*, New York: State University of New York Press.

'A Realist Theory for Economics', in R.E. Backhouse (ed.), *New Directions in Economic Methodology*, 1994, London and New York: Routledge.

'Why Are So Many Economists So Opposed to Methodology?', *Journal of Economic Methodology*, 1994, Vol. 1, No.1, pp. 105–133.

'The "Lucas Critique": A Generalisation', *Cambridge Journal of Economics*, 1995, Vol. 19, No. 2, pp. 257–277.

'A Realist Perspective on Contemporary "Economic Theory"', *Journal of Economic Issues*, 1995, Vol. 29, No. 1, pp. 1–32.

Tony Lawson
Cambridge
January 1996

Part I

SCIENCE AND ECONOMICS

1

ENDURING TENSIONS AS POINTS OF DEPARTURE

Contemporary academic economics is not in a healthy state. Over many years now problems have regularly come to light which throw considerable doubt on the capacity of many of its strands to explain, or even always to address, real world events or to facilitate policy evaluation. Such problems especially beset the rather dominant 'mainstream' or 'orthodox' project,[1] centring on econometrics and formalistic 'economic theory', which is my main concern here. This unhappy situation, moreover, appears to be increasingly recognised both inside and outside of the academy.

On the outside, for example, the (UK) *Observer Magazine* (20 September 1992: 7) concludes that 'there's no such thing as economics. It's all voodoo, bluff and pseudo-science'. *New Scientist* (31 October 1992: 26–31) even carried sketches of economists forecasting economic variables by reading lines on the palms of their hands and of econometricians pushing numbers around while blindfolded. Donaldson (1984) sums up this sentiment in reporting 'a growing suspicion that in fact economists may not be very good at dealing with the real problems which face us' (Donaldson, 1984: 11). It seems that the 'charge against economists is, above all, that of irrelevance' (ibid.: 13).

Within the economics' academy, the unsatisfactory nature of the prevailing state of affairs appears increasingly to be acknowledged by non-orthodox and orthodox economists alike. In truth, non-orthodox economists have long recognised and emphasised it (see, for example, references in Hodgson, 1988). It is noteworthy, however, that those mainstream economists who reflect upon the nature and state of the discipline are increasingly also found to be airing some concern. The field of econometrics, for example, has witnessed a stream of contributions with such titles as 'Econometrics – Alchemy or Science?' (Hendry, 1980) or 'Lets take the con out of Econometrics' (Leamer, 1983). In the latter Leamer observes that 'after three decades of churning out estimates, the econometrics club finds itself under critical scrutiny and faces incredulity as never before' (p. 42), and goes so far as to suggest

that 'hardly anyone takes anyone else's data analyses seriously' (p. 37). At the same time 'economic theory' or 'pure theory', that most prestigious strand of contemporary economics, including its traditional exemplar general equilibrium theory, recently seems also to have been recognised as a project in dire straights. The upshot here is a bout of articles with such titles as 'The Intrinsic Limits of Modern Economic Theory: The Emperor has no clothes' (Kirman, 1989), or books with such titles as *Economics in Disarray* (Wiles and Routh (eds), 1984), *The Crisis in Economic Theory* (Bell and Kristol, 1981) or 'The Death of Economics' (Ormerod, 1994), and commentaries by 'pure theorists' such as Hahn (1991) effectively acknowledging the demise of his long term hopes of successes for the project. One Nobel Memorial Prize winner in economic science despairs of the whole contemporary project, viewing it as basically an arbitrary activity which singularly fails to advance understanding:

> Page after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions.... Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system.
>
> (Leontief, 1982: 104)

Needless to say some diagnoses of this situation have been produced by those working within the mainstream project and certain 'solutions' proposed. But none have proven to be successful. I shall argue that this failure reflects an inadequate analysis of the nature of the disorder. The 'solutions' so far tried, indeed, have achieved little more than a modification in the form of the symptoms, and in certain cases might themselves be interpreted merely as novel manifestations of the underlying problems. An aim of this book then is to attempt to elaborate the real nature of the disorder afflicting contemporary economics with a view to identifying an appropriate course of action.

Now when any project falls into a state of disarray, signs of disorder tend to be manifest in additional ways to failure in terms of the criteria which the project sets itself. This is so with modern economics, and in particular with the extraordinarily influential mainstream project which is my primary concern here. The most widely noted cause for concern is that while economic forecasters do not forecast sufficiently accurately, 'theorists' fail to provide non-arbitrary explanations of any real economic

phenomenon of interest. But at least as significant are widespread and persistent tensions and incongruities that are best described as theory/practice inconsistencies. In contemporary economics, there are many situations in which economists claim, in their theorising, to be doing one thing, when it is quite evident from their practices, and acknowledged by the more reflective members of the community, that they are actually doing something quite different. In the light of the confusions and uncertainties which such theory/practice inconsistencies can generate, resulting in unhappy compromises, *ad hoc* accommodations, the explicit suppression of criticism and other pathologies of action, the existence of these inconsistencies is at least as significant as the shortcomings in actual results.

Although the inconsistencies between theoretical perspectives and actual practices that I have in mind are numerous and span all aspects of academic economic research, it is sufficient for my purposes to identify but a few, albeit widely prevalent and enduring, forms. Specifically, I shall briefly sketch three types, relating, in turn, to the implementation of method, social theory and methodology. These examples should be sufficient to indicate the scope and nature of the problems. They also make it apparent that any programme of transforming the discipline into something rather more useful than it has been over the last fifty years or so will need to transcend (and explain) enduring contemporary incoherences and problems that are somewhat more wide ranging than the inability of the contemporary project to forecast or explain any actual economic event or situation.

INCONSISTENCY AT THE LEVEL OF METHOD

A first disparity that I want to consider, one that is often commented upon (e.g. Blaug, 1980; McCloskey, 1983, 1986), is that economists frequently employ methods, practices and techniques of enquiry and modes of inference, that are inconsistent with the theoretical perspectives on method which they claim to draw upon. As McCloskey (1983) interprets the situation:

> Economists do not follow the laws of enquiry their methodologies lay down. A good thing, too. If they did they would stand silent on... [most of the]... matters about which they commonly speak. In view of the volubility of economists the many official methodologies are apparently not the grounds for their scientific conviction.
>
> (McCloskey, 1983: 482)

In other words, according to McCloskey, economists 'have two attitudes towards discourse, the official and unofficial, the explicit and the

implicit. The official rhetoric, to which they subscribe in the abstract and in methodological ruminations, declares them to be scientists in the modern mode' (ibid.: 484). But it is not a rhetoric, in McCloskey's assessment, that fits their actual practices.

Econometrics is the paradigm example here. It is widely observed, for example, that when econometricians formulate and (often simultaneously) estimate their 'models' they frequently 'run' hundreds if not thousands of regressions, and in doing so contradict the classical theory of inference they explicitly acknowledge. Moreover, when their models are used to forecast unobserved (typically future) states of the economy, econometricians repeatedly make *ad hoc* revisions to estimated parameter values, or introduce 'add on' factors, in order to generate results that are 'sensible' or 'believable', thereby contravening what Lucas designates the 'theory of economic policy' (Lucas, 1976). Thus although economists do pay 'lip-service' to this latter theory Lucas identifies 'many signs that practicing econometricians pay little more than lip-service to the theory' (ibid.: 22). It is this observed theory/practice inconsistency, indeed, which motivated the extraordinarily influential 'Lucas critique' of economic policy evaluation (ibid.). In this critique (which we shall cover in Chapter 7) Lucas focuses specifically upon the 'indifference of econometric forecasters to [certain sub-sets of available] data series'; 'the frequent and frequently important refitting of econometric relationships'; and 'most suggestively... the practice of using patterns in recent residuals to revise intercept estimates for forecasting purposes' (ibid.: 22, 23), where all such practices are inconsistent with accepted theory.

It is not only outsiders to the econometrics community who draw attention to these and related matters. Indeed, one of the most telling assessments is provided by Leamer (1978) who is himself a respected theoretical and practising econometrician. Leamer identifies a clear gap between econometric theory and practice. Moreover, in doing so he draws attention to the calmness with which most econometricians accept this situation:

> The opinion that econometric theory is largely irrelevant is held by an embarrassingly large share of the economics profession. The wide gap between econometric theory and econometric practice might be expected to cause professional tension. In fact, a calm equilibrium permeates our journals and our meetings. We comfortably divide ourselves into a celibate priesthood of statistical theorists, on the one hand, and a legion of inveterate sinner-data analysts, on the other. The priests are empowered to draw up lists of sins and are revered for the special talents they display. Sinners

> are not expected to avoid sins; they need only confess their errors openly.
>
> (Leamer, 1978: vi)

In fact the situation is further puzzling. For those who teach econometric theory and those who practice econometric modelling not only employ identical rhetoric (the rhetoric of the theory that supposedly grounds the officially sanctioned methods of enquiry), they also usually constitute the same individuals flitting between the noted incompatible activities. Consider Leamer's experiences once more:

> I began thinking about these problems when I was a graduate student in economics at the University of Michigan, 1966–1970. At that time there was a very active group building an econometric model of the United States. As it happens, the econometric modelling was done in the basement of the building and the econometric theory courses were taught on the top floor (the third). I was perplexed by the fact that the same language was used in both places. Even more amazing was the transmogrification of particular individuals who wantonly sinned in the basement and metamorphosed into the highest of high priests as they ascended to the third floor.
>
> (ibid.: vi)

What can be made of all this? We can allow that theory/practice inconsistencies *per se* are characteristic of all formative, learning and/or developmental processes. They are a pointer that, for (further) progress to be achieved, the existing framework of analysis must be transformed in some way. If, for example, theory cannot be followed in practice, or if practice achieves something which in theory is impossible, then the theory, at least, stands in need of revision. As any research project progresses the only sort of consistency over time that can conceivably be sustained, or reasonably aspired to, or expected of it, is of a developmental sort, much like a tadpole evolving into a frog or initial thoughts and jottings being transformed into a thesis. The real failing of contemporary econometrics that Leamer is observing, however, is the preparedness of so many econometricians to live with the observed incongruities, and thereby to sanction the proliferation of compromise formulations, *ad hoc* stratagems and the like, which inevitably result.

Leamer himself, it should be emphasised, is not guilty of this. Indeed, his book, which is premised on the belief that it 'is a sin to not know why you are sinning', represents a desire to 'discard the elitism of the statistical priesthood and proceed under the assumption that unavoidable sins cannot be sins at all' (ibid.: vi). Ultimately, however, he recognises that his own proposed solution, of replacing the classical

theory with the Bayesian theory of inference, can achieve only limited success, with the result that Leamer is rather pessimistic about the prospects:

> Nor do I foresee developments on the horizon that will make any mathematical theory of inference fully applicable. For better or for worse, real inference will remain a highly complicated, poorly understood phenomenon.
>
> (ibid.: vi)

But if a mathematical theory of inference is inapplicable to economic phenomena it does not follow that a satisfactory and comprehensible alternative is beyond reach. Certainly a feasible alternative hypothesis worth contemplating is that a somewhat more radical response than a shift from the classical to the Bayesian approach is required. Moreover, given that econometricians appear to decide which equation specifications including parameter estimates and/or model extrapolations are acceptable prior to any formal econometric exercise, the question naturally arises as to whether a formal method of statistical inference is really required at all. In any case, I shall demonstrate in due course that a radical transformation in the over-all orientation of the subject can indeed succeed in transcending problems of the sort in question. Before turning to such matters explicitly, though, some further examples of practices which are inconsistent with the theories which supposedly ground them, can be noted. For it is not only in the labours of econometricians that we find theory and practice to be persistently out of step.[2]

INCONSISTENCY AT THE LEVEL OF SOCIAL THEORY

Other such tensions or inconsistencies turn on, or, more accurately, are manifest at the level of, the substantive *content* of economic theories. Specifically, in their day-to-day discussions, generalised rhetorics, public and/or programmatic stances, mainstream economists refer to many substantive items which their official ways of proceeding cannot accommodate without severe strain. Such items even include the basic economic categories of choice, institutions, markets, money, social relationships, uncertainty and change (see e.g. Farmer and Matthews, 1991: 106). I focus here on just one of these. Most economists appear to acknowledge an intuition that human beings possess the capacity of exercising real choice. Not only does the language of choice come into all the day-to-day musings of economists and figure in their programmatic stances, but mainstream economists frequently even refer to their project, or to a prominent strand of it at least, as *choice theory*. So prevalent is the rhetoric of choice, indeed, that Duesenberry (1960: 233) famously

concludes that what distinguishes the subject (from sociology) is that '[e]conomics is all about how people make choices'.[3]

Yet it isn't. In the formal 'models' found in mainstream journals and books, human choice is ultimately denied. For if real choice means anything it is that any individual could always have acted otherwise. And this is precisely what contemporary 'theorists' are unable to allow in their formalistic modelling. That is, although the reality of choice appears to be widely acknowledged by economists in their more informal discussions and public pronouncements, the exercise of choice is a phenomenon that is always absent from the formal substantive analyses that are conventionally reported. Instead, individuals are represented in such a way that, relative to their situations, there is almost always but one preferred or rational course of action and this is always followed. Despite some suggestive rhetoric, human doings, as modelled, could not have been otherwise. (On this also see Shackle, 1972, 1989; Loasby, 1976; Farmer, 1982; Hodgson, 1988, 1993a; Runde, 1997.)

The disparity in question here, as with the inconsistencies of econometric theory and practice discussed above, is widely recognised in mainstream contributions. In this regard attention is often drawn to 'dilemmas', 'conundrums', and the like. But few resources are devoted to resolving the matter. The usual resort, it seems, is to hope that the noted tension will somehow disappear of its own accord. In practice, ways of coping are usually arbitrary. A familiar enough example of this is the partitioning of postulated econometric relationships into endogenous and exogenous components, with only the latter being regarded as subject to choice, albeit in the exercise solely of a privileged policy-making elite. But, no sooner is this latter assignment of the possibility of choice made than it is undermined by the remorseless tendency of that same project to endogenise ever more variables. A recent example of this lies in the efforts of proponents of the rational expectations hypothesis to convince other mainstream economists to accept, and to produce models consistent with, the claim that all agents' beliefs are (in the sense of that hypothesis) rational. A version of the dilemma or 'conundrum' which thereby arises is noted by Sargent and Wallace (1976):

> The conundrum facing the economist can be put as follows. In order for a model to have normative implications, it must contain some parameters whose values can be chosen by the policy maker. But if these can be chosen, rational agents will not view them as fixed and will make use of schemes for predicting their values. If the economist models the economy taking these schemes into account, then those parameters become endogenous variables and no longer appear in the reduced form equations for the other

> endogenous variables. If he models the economy without taking the schemes into account, he is not imposing rationality.
>
> (Sargent and Wallace, 1976: 183)

This puzzle for orthodox economics is alternatively put by Reder (1982):

> Associated with the assumption of stable preferences, but logically distinct, is the 'thrust for endogenisation'. A leading manifestation of this tendency is Stigler's attempt to explain – and constrain – the behaviour of political decision makers, but it is not the only one...
>
> Successfully to endogenize a new variable is to enhance the explanatory power of economics, and there is much interest in such achievements. However, it must be noted that where variables are made 'endogenous', they can no longer serve as objects of social choice.
>
> To the extent that variables are endogenized – choice is explained – 'society's' freedom of choice is seen as illusory. Freedom appears to consist not in power of choice, but (*pace* Hegel) in recognition of necessity. This is not a likely conclusion for followers of Adam Smith, and surely not one they desire, but one from which they can be saved only by failure of this direction of research.
>
> (Reder, 1982: 34–5)

We might notice, too, that the knowledge process as represented in mainstream formulations, is always a deterministic and in fact quite individualistic affair, wherein each agent unproblematically records economic 'signals' or other atomistic items of information. Usually, the latter signals take the form of market prices. But, in general, any measurable event or state of affairs is a potential candidate. All that appears to be essential in this process is that agents be passive sensors, that is, unreflexive receivers and recorders of whatever 'signals' are deemed to be available.

The overall result is a conception wherein any contribution of human beings to the active making of their own history is denied. Indeed, the continuing tendency of mainstream economics to characterise human agents as passive automata, propelled along under the influence only of external forces, invites here a restatement of Veblen's so colourful and apt enduring assessment of this, usually 'hedonistic', person:

> The hedonistic conception of man is that of a lightning calculator of pleasures and pains, who oscillates like a homogeneous globule of desire of happiness under the impulse of stimuli that shift him about the area, but leave him intact. He has neither antecedent nor consequent. He is an isolated, definitive human datum, in stable equilibrium except for the buffets of the impinging forces that displace him in one direction or another. Self-imposed in elemental

> space, he spins symmetrically about his own spiritual axis until the parallelogram of forces bears down upon him, where upon he follows the line of the resultant. When the force of the impact is spent, he comes to rest, a self-contained globule of desire as before. Spiritually, the hedonistic man is not a prime mover. He is not the seat of a process of living, except in the sense that he is subject to a series of permutations enforced upon him by circumstances external and alien to him.
>
> (Veblen, 1919: 73, 74)

The dilemma for mainstream economists, then, is whether to abandon the widely felt intuition that human choice is real *or* to stick with it and forego methods and associated theories which ultimately cannot accommodate it. Formally, in contriving formalistic 'models' and insisting that this is the only 'proper' way to proceed (even if 'unofficial practices' may be rather different) mainstream theorists settle for the former option. Quite clearly, though, if choice is a real feature of human agency that is continually exercised we ought to be doing something rather better than sanctioning practices or theories (of explanation as well as human doings and situations) in which it ultimately cannot be sustained.

INCONSISTENCY AT THE LEVEL OF METHODOLOGY

A third tension that can be mentioned here concerns the prevalent orientation to methodology. While mainstream economists frequently conclude that 'methodology' or 'philosophy' is irrelevant to (progress in) economic science, and actively discourage it, these very same economists appear quite unable to refrain from explicit methodological discussion and enquiry themselves (see e.g. Backhouse 1992; Hahn, 1992a, 1992b). Methodology, it seems, is regarded as being useless to, and a distraction from, economic science in theory and yet indispensable for it, or anyway unavoidable, in practice.

Part of the puzzle here is why it should ever be thought that methodology could be dispensable in theory. Indeed, it will be necessary to account for this theoretical perspective in due course. For the time being, however, it can be noted that such a theoretical orientation is pervasive, being manifested in many ways. Perhaps the most immediate form is the perpetual repetition of such quips as 'don't think about it just do it'; or 'methodologists are crazy' or 'those who can, do economics while those who cannot, do methodology'.[4] An effective restraint on methodology, moreover, is the clear reluctance of mainstream economic journals to publish much of it. Also significant is the apparent refusal of many central research funding authorities to promote it. In the UK, for

example, the training currently provided and recommended for economics students tends to be more or less devoid of any explicit methodological content. A quick run through a recent Economic and Social Research Council[5] *Guidelines for Post Graduate Training* reveals that economics is one of only two out of the twenty subject guidelines provided (the other being for 'town planning') that do not include an explicit section detailing the need for some form of formal training in the 'philosophy of the social sciences' or something similar.

But this opposition to methodology is not (and could hardly be) a coherent or consistent one.[6] Not only do those who most vociferously oppose methodology repeatedly fall back upon it in their own substantive contributions, but every now and again, if mainly in coffee-room discussions or local seminars, they can be found putting forward arguments and defending positions that can only be described as overtly methodological/philosophical. For example, Frank Hahn has both published on methodology (see for example Hahn, 1984, 1985) and, in my own institution, frequently entered into local methodological debate. Yet, his official position has long been one of opposition to training or study in such matters. Thus when in the July 1992 issue of the *Royal Economic Society Newsletter*, Backhouse puts the question: 'Should we ignore methodology?', the heading of a response by Hahn is 'Answer to Backhouse: Yes'. The interchange which follows is itself a response to a contribution by Hahn in the earlier, April 1992, *Newsletter* whereby, on the occasion of his retirement from Cambridge, Hahn offers various 'reflections' which take the form of advice to young economists. Notably these include the recommendation to 'avoid discussion of "mathematics in economics" like the plague', and [as if unrelated] to 'give no thought at all to methodology'. When Backhouse, in the later July issue of the *Newsletter* points out that Hahn too practices methodology, Hahn's grudging acknowledgement of this observation is still tempered by an explicit astonishment that anyone could think that economists should *learn* philosophy, or even spend much time on philosophy/methodology at all:

> Methodology like original sin won't go away, and Backhouse is right in saying that I myself have sinned. Perhaps it would have been better if I had not.... What I really wanted to advise the young to do was to avoid spending much time and thought on it. As for them learning philosophy, whatever next?
>
> (Hahn 1992b)

It perhaps warrants some emphasis here that orthodox economists do not just formally discourage methodology, they do so explicitly and boldly. They do not so much neglect the topic (as, say, they often neglect real world structures and abuses of power, widespread wars, famines

and other miseries, social decline) they boldly assert, as Hahn does, that any engagement in methodology is a waste of time. Moreover, they do so, by and large, without much explicit or cogent argument. The most that tends to be offered by way of persuasion are rather bland assertions. It is as if the antagonists hardly feel it is worth bothering to think about the question anyway. Caldwell (1990: 64; see also 1993: 45) records a similar observation.

> At the 1989 History of Economics Society meetings in Richmond, Virginia there was a session entitled, 'Should Methodology Matter to the Economist or to the Historian of Economics?' Some of the participants answered in the negative. As an observer I was disappointed in the session, not because the study of methodology was attacked, but because the attack was such an anaemic one. The major worry seemed to be that many economists think that methodological study is a waste of time. One panellist even suggested that it would be alright to keep doing methodological investigations as long as we called them by another name so as not to offend our fellow economists.
>
> Lest there be any doubt, it should be stated at the outset that, at least in the US, most economists are indifferent towards methodology, and many of the rest are openly hostile to it. Indeed, *explaining* these attitudes is a methodological topic worthy of further study. But their existence, at least for now, should be taken as a given.
>
> (Caldwell, 1990: 64)

Now there may be some validity to Hahn's impression that methodological discussion within economics has often proven *ex post* to be largely unfruitful. But, if this is so, the widespread *a priori* assessment that methodology cannot bear fruit, as we shall see below, is not one that thereby follows, nor one that has any validity at all. It is merely a claim, to repeat, that is never really argued but asserted. However, in their failure to articulate cogent arguments mainstream economists, I suspect, are not choosing to be arbitrary or to discriminate randomly. Rather, to such people the dismissal of methodology appears to be a matter of common sense, an intuition that lies naturally with more generally held belief systems and mentalities. In consequence, the dismissal takes on the appearance of being philosophically neutral. It is put forward as the obviously sound attitude for any economist to take, at one with the project of economics, and well captured in such remarks as 'don't think about it just get on with it'. To repeat, however, it represents a stipulation that economists are unable to live up to. And the point here is that the coincidence of a widespread acceptance of this stipulation in economics with an obvious inability to act consistently with it, is a further phenomenon that warrants some explanation, a situation which once

more suggests an undesirable level of confusion and inconsistency in the contemporary economics discipline.

In summary, contemporary economics is not in a fit state. Most obviously, it fares poorly on its own terms; it neither provides particularly accurate forecasts of events nor illuminates the world in which we live. But of equal significance, the whole project is riddled with confusion and incoherence. Rarely, though, is the significance of this wider situation acknowledged. Certainly the persistent theory/practice inconsistencies focused upon here will be recognisable to most practising economists. But of those who work in, or who address, the mainstream, only a few (Blaug, Hendry, Leamer, Lucas, McCloskey, Reder and the like) seem prepared to treat these inconsistencies with anything like the sort of seriousness they warrant. Rather the dominant tendency is to regard the noted puzzles or tensions in the manner of mild irritants that can be lived with, if necessary, but which are expected, in due course, somehow to wither away of their own accord. Thus, the failure of the contemporary project to explain, predict or otherwise illuminate is met with only a restricted set of responses: with continuous revisions to certain parts of theories, the collection of ever more data, the development of increasingly sophisticated forms of computer software, and so forth.

While such developments are usually welcome ones, in the main they do nothing to address, let alone resolve, problems, tensions, confusions and consequent pathologies of action, which turn on the acceptance of enduring discrepancies between practices adopted and the theories which are supposed to inform them. All such responses, rather, share the feature of allowing, in their pursuit, that the existing ways of doing economics by and large remain untouched. Once, however, we face up to the depth and extent of the problems that pervade the discipline it is apparent that some more thorough-going response is warranted. And, amongst other things, it is evident that a rather fundamental, necessarily broadly methodological, enquiry, and a sustained one at that, is called for. In what follows, in fact, it will be seen that a resolution of the noted inconsistencies, along with other more general limitations of the contemporary discipline, necessitates a quite radical transformation in the very nature of contemporary economics.

2

REALISM, EXPLANATION AND SCIENCE

How are the problems and failings of modern economics to be explained and resolved? The answer developed here is that these problems: (1) result ultimately from a widespread, rather uncritical, reliance by economists upon a questionable conception of science and explanation; and (2) can be resolved through replacing this conception with a more adequate one, derived by way of adopting an explicitly *realist* orientation. The next few chapters are concerned with elaborating upon this 'answer'. In the process I indicate just how adopting a realist orientation (in the sense understood here) can facilitate a more relevant economics than is currently available or is even possible within the confines of the explanatory norms and criteria of the contemporary economics discipline. It is not until Chapter 4, however, that I deal fully with the problems and/or inconsistencies just noted.

REALISM

I start by describing the sort of position that I am designating realist. In fact any position might be designated a *realism* (in the philosophical sense of the term) that asserts the existence of some disputed kind of entity (such as black holes, class relations, economic equilibria, gravitons, tanon-neutrinos, utilities). Clearly on this definition we are all realists of a kind, and there are very many conceivable realisms. In science, a realist position, i.e. a *scientific realism*, asserts that the ultimate objects of scientific investigation exist for the most part quite independent of, or at least prior to, their investigation. Now the conception of realism I want to argue for is closely and explicitly bound up with *ontology* or 'metaphysics', i.e. with enquiry into the nature of *being*, of *existence*, including the nature, constitution and structure of the objects of study. Indeed, it is a forthright concern with ontology, and in particular with elaborating the broad nature of aspects of natural and social reality, that explains, in what follows, the term realism being used in labelling perspectives distinguished. First and foremost, then, the term realism is

utilised here to denote specific accounts of the nature of reality, natural and/or social.

But why bother with realism so interpreted? Surely economists can and should avoid wasting their time reflecting upon such matters as 'realism' and 'ontology', and, accepting the goals, criteria, methods and procedures as laid down in standard economic texts books and recent articles, just get on with the job? Even accepting that contemporary economics has proven to be notoriously unsuccessful over the last fifty years or so, is not the proper solution just to continue as before, albeit trying that little bit harder?

The latter has been the usual and recommended response, of course, at least within the mainstream. The problem with it, we shall see, is that the consequent disregard for the nature of social material has in practice resulted in certain methods and procedures being accepted as fundamental to 'proper' economics, and even held as more or less universally valid, that are actually quite inappropriate to the analysis of social objects.[1] In propagating their project contemporary orthodox economists seem merely to assume that whatever the methods employed, social reality must, as it were, 'fit'. We will find that it is this presumption that ultimately lies behind the problems of the discipline noted in the previous chapter. It is thus with the intention of avoiding such an epistemological reductionism, and in particular of facilitating ways of proceeding that are adequate to social objects, that ontology is an explicit concern here. In other words, a realist orientation of the sort I am intending to defend insists that methods of social science can, and indeed should, be designed to take account of available insights concerning the nature of social material. To repeat, it will be seen that it is by way of adopting such a strategy that the noted problems of the discipline can be resolved.

DEDUCTIVISM

But is there really a set of methods or ways of proceeding in economics that can be shown to be both as fundamental to the mainstream project as I am suggesting, and yet largely irrelevant to the analysis of social phenomena? Even accepting the often noted incapacity of orthodox economics to explain or otherwise illuminate real world phenomena, surely it is impossible to lay the 'blame' for this at the door of any erroneously universalised set of methods or modes of analysis? Actually, I think it is possible. The main 'culprit', I shall argue, is a mode of explanation that can be referred to as *deductivist*, or, more particularly, it is the conception of 'laws' (or 'significant results' or 'theoretical formulations') upon which deductivist explanation ultimately depends.

This conception of laws[2] is formulated in terms of constant

conjunctions of events or states of affairs. It is an interpretation of laws as, or as dependent upon, constant relations connecting outcomes at the level of the actual course of events or states of affairs. On this view, laws, which are often referred to as 'covering laws', express regularities of the *form* 'whenever event x then event y'. This formulation should be interpreted quite generally. Thus, 'event x' can be a composite of many events, for example, and the suggested relationship between events can be probabilistic (so that y can be interpreted as the average or limit of a series) or deterministic. In practice, the noted perspective on laws is usually associated with the principle that such laws are to be assessed (confirmed, corroborated, falsified, tested) by their instances. By deductivism I simply mean the collection of theories (of science, explanation, scientific progress, and so forth) that is erected upon the event regularity conception of laws in conjunction with the just noted principle of theory assessment.

Now, generally speaking, to explain some event, thing, or phenomenon, (i.e. the 'explanandum') is to provide an account (the 'explanans') whereby the initial phenomenon is rendered intelligible. According to deductivist explanation, then, the explanandum must be deduced from a set of initial and boundary conditions plus universal laws of the form 'whenever event x then event y'. Clearly on this deductivist conception, explanation and prediction amount to much the same thing (this is the so-called symmetry thesis). The former entails the deduction of an event after it has (or is known to have) occurred, the latter prior to (knowledge of) its occurrence.[3] According to deductivism, the explanation of laws, theories and sciences similarly proceeds by deductive subsumption.

We can note, parenthetically, that this theory of explanation is also variously known as the covering law model, the Popper–Hempel theory of explanation (Popper first restated it in recent times, Hempel has been its most ardent defender), the deductive-nomological model (*nomos* being Greek for law) or D-N model for short, amongst other things. In the case of 'statistical laws' the D-N description is often replaced by the 'inductive-probabilistic' or I-P label, though many include both under the former deductive-nomological head.

The deductivist model of explanation is easily illustrated. Suppose I want to use the D-N model to explain the event that my car radiator contained ice this morning. I need a set of initial conditions and at least one universal 'covering law' which logically entails this event. A possible contender for an explanation comprises the following set of *initial conditions*: (1) my car radiator contained water yesterday, (2) my radiator does not leak, (3) the temperature fell below 0°C last night, along with the *empirical law*: water freezes at 0°C.[4]

In practice this sort of explanation is frequently laid out as follows:

1 Initial conditions
2 Laws or theories

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3 Phenomenon explained and/or predicted

Thus, the example just given can be structured in the following manner:

I1 My car radiator contained water yesterday
I2 My car radiator does not leak
I3 The temperature fell below 0°C last night
L1 Water freezes at 0°C

P1 My radiator contained ice this morning

I do not think that it is contentious to observe that deductivism so understood, and in particular the conception of laws which underpins it (however the event regularities might be labelled or interpreted), characterises contemporary mainstream economics. Just as the persistent search for event regularities of a probabilistic kind characterises econometrics for example (see Chapter 7), so the positing of strict constant event conjunctions, interpreted usually as 'axioms' or 'assumptions', is a condition of modern 'economic theorising'[5] (see Chapters 8 and 9). Rather, it is implicitly taken for granted that the deductivist model of explanation is universally valid. Those few, typically non-orthodox, economists who occasionally voice a degree of concern at the extent to which mainstream economists rely upon the deductivist conception are usually (mis)interpreted as not agreeing with the particular substantive premises, the axioms or assumptions, that are proposed. Criticism of the mode of reasoning is something that appears rarely even to be contemplated within the mainstream; the relevance of deductivism is never questioned. As Hahn (1985) writes:[6]

> Opponents of [economic] theory often argue that it is tautologous because it consists of logical deductions from axioms and assumptions. If one is kind to such critics one interprets them as signalling that they do not care for these axioms and these assumptions. In any case all theory in all subjects proceeds in this manner.
>
> (Hahn, 1985: 9)

Here, though, I do want to question the presumption of the generalised relevance of deductivism. In particular, I want to determine the conception of reality presupposed by it, and specifically by the theory of laws or law-like statements on which it depends, and to assess the relevance of this conception as a characterisation of social phenomena. My aim, in other words, is to determine the character of social material